pass south of the sun, it will not be so easy to use buildings as screens in the northern hemisphere, and special means must be devised. The luminous ring will be as bright and conspicuous as in 1866, and the first appearance of the prolongation of the cusps may be looked for about the 24th of November.

It is now evident that similar opportunities will happen on the 1st of December, 1898, when least distance of centres will be about 1°, and about the 28th of November, 1906, when least distance of centres will be about 1½°, the planet in both cases south of the sun. In each case the least distance of centres will be less than the limit within which the formation of the luminous ring is possible, but the duration of the ring will be successively less as the least distance between centres becomes greater. No other opportunities will present themselves until near the end of the next century, when they will occur in June.

Similar opportunities must have occurred in years preceding 1866; that is, on the 14th of December, 1858, and also on the 16th of December, 1850; but it does not appear that either was used. This last date is only nineteen months after Mädler's observations in May, 1849; and, if any one properly situated as to time had endeavored to repeat Mädler's observation on the day of conjunction, he would almost certainly have seen the luminous ring.

LEWIS R. GIBBES.

Charleston, S.C., Nov. 13.

A Problem in Physics.

AN experiment was tried by Joule nearly fifty years ago which has attained a world-wide reputation, and which has crept into nearly every text-book of physics. The commonly accepted interpretation of it, however, would seem not entirely satisfactory. I will quote from Tait's description of the experiment.

"Joule took a strong vessel containing compressed air, and connected it with another equal vessel which was exhausted of air. These two vessels were immersed each in a tank of water. After the water in the tanks had been stirred carefully, . . . a stopcock in the pipe connecting the two vessels was suddenly opened. The compressed air immediately began to rush violently into the empty vessel, and continued to do so till the pressure became the same in both; and the result was, as every one might have expected, that the vessel from which the air had been forcibly extruded fell in temperature in consequence of that operation. It had expended some of its energy in forcing the air into the other vessel; but that air, being violently forced into the other vessel, impinged against the sides of that vessel, and thus the energy with which it was forced in through the tap was again converted into heat. On stirring the water round these vessels, after the transmission of air had been completed and the stop-cock closed, Joule found that the number of units of heat lost by the vessel and the water on the one side was almost precisely equal to the quantity of heat which had been gained on the other side." Tyndall gives the following (let B represent the vessel in which the air was compressed to 22 atmospheres, and A the vessel which was exhausted): —

"Now, the air, in driving its own particles out of B, performs work, . . . and the air which remains in B must be chilled. The particles of air enter A with a certain velocity, to generate which the heat of the air in B has been sacrificed; but they immediately strike against the interior surface of A, their motion of translation is annihilated, and the exact quantity of heat lost by B appears in A. The contents of A and B mixed together give air of the original temperature. There is no work performed, and there is no loss of heat." Tyndall gives an illustration of a cylinder having a piston in the centre, and the space above the piston a vacuum. Suppose the air below the piston is heated up from 0° to 273° C. "If the pressure were removed, the air would expand, and fill the cylinder. The lower portion of the column would thereby be chilled, but the upper portion would be heated; and, mixing both portions together, we should have the whole column at a temperature of 273°. In this case we raise the temperature of the gas from 0° to 273°, and afterward allow it to double its volume. The temperatures of the gas at the beginning and at the end are the same as when the gas expands against a constant pressure, or lifts a constant weight; but the absolute quantity of heat in the latter case is 1,421 times that employed in the former, because, in the one case, the gas performs mechanical work, and in the other not."

The following quotation is from Balfour Stewart, and bears upon this question: —

"The prevalent idea is, that when air expands it becomes colder, and that when condensed it becomes hotter; but Joule, by experiment, has shown that no appreciable change of temperature occurs when air is allowed to expand in such a manner as not to develop mechanical power. It follows as an inference, that, when air is compressed, the rise of temperature is scarcely at all due to the mere diminution of the distance between the particles, but almost entirely to the mechanical effect which must be spent on the air before this condensation can be produced."

A final quotation is taken from Ganot's "Physics:"—

"A strong metal box is taken, provided with a stop-cock, on which can be screwed a small condensing-pump. Having compressed the air by its means as it becomes heated by this process, the box is allowed to stand for some time, until it has acquired the temperature of the surrounding medium. On opening the stop cock, the air rushes out; it is expelled by the expansive force of the internal air: in short, the air drives itself out Work is therefore performed by the air, and there should be a disappearance of heat; and, if the jet of air be allowed to strike against a thermopile, the galvanometer is deflected, and the direction of its deflection indicates a cooling. . . . Joule placed in a calorimeter two equal copper reservoirs, which could be connected by a tube. One of these contained air at 22 atmospheres; the other was exhausted. When they were connected, they came into equilibrium under a pressure of 11 atmospheres; but, as the gas in expanding had done no work, there was no alteration in temperature."

I have given these quotations rather freely from standard authors, in order to present the problem as clearly as possible. In order to arrive at just the action taking place in this experiment, it seems to me a phenomenon first described by Faraday in 1827 should be mentioned. Gas compressed to 30 atmospheres was allowed to suddenly enter a cylinder 30 feet long, in which the gas was at atmospheric pressure presumably. It was found, that, where the gas rushed in, the cylinder was much cooled, while at the other end it was heated. It would seem that in this case the heating was not produced by the particles of gas impinging upon the end of the cylinder. If a piston were placed in front of the expanding gas, the whole of the gas on the other side of the piston would be compressed and heated. If, now, instead of a piston, we open a stop-cock at the end of the cylinder, the gas would stream in and compress that already there, and heat it; but the gas, expanding violently as it enters, would be much cooled, and this would more than counteract the heating where it enters. Thus the farther end would show a heating, while the end at the orifice would show a cooling as observed. Have we not precisely analogous phenomena in Joule's experiment? For a very small fraction of a second (perhaps .0001) after the stop-cock was opened, there would be a partial vacuum in A, into which the air streams; but after that the particles would not impinge upon the sides of A, but would have their velocity diminished and finally overcome by striking other particles. In imparting this velocity, the particles in B would be slightly chilled. The air, in streaming out of B, would be cooled by expansion after an instant, and would serve to cool the end of A near the orifice, as we have just seen; also the chilled particles in B would stream into A, and thus cool it still more. Whatever may be the action in these vessels, it is certain that the final heating in A, and cooling in B, would be exceedingly slight as shown by Joule's experiment, though it does not seem that the popular explanation is entirely correct.

It seems to me this question of the action of air in Joule's two vessels is an intensely interesting one. The conclusion that the chilling of the air in the vessel due to the work of imparting a velocity to its particles is very slight, corroborates in a marked manner the experiments tried by the present writer, in which he found a cooling of four degrees, while the dynamical cooling should have been ten times greater. The quotation from Ganot shows precisely an analogous case.

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Washington, Nov. 17.